Earth Surf. Dynam. Discuss., 3, C273–C277, 2015
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Earth Surface Dynamics
Discussions
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# *Interactive comment on* "The periglacial engine of mountain erosion – Part 2: Modelling large-scale landscape evolution" *by* D. L. Egholm et al.

**D. L. Egholm et al.**

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Received and published: 14 August 2015

In this reply we comment on all remarks given by the reviewer and present the associated changes to the manuscript. The comments from each review have been copied into this document in grey and are marked with C for comment and a sequential number. The corresponding response is marked with R.

**Reviewer 4: T. Hales**

This paper presents a numerical study that explains the development of flat summits that are common in areas with significant periglacial action. This is a significant gap in the literature and I found the approach to be novel. The paper is well constructed, and I particularly liked the introduction that explains the state of knowledge well. In



short, I really like the theory. C-4.1: There are two areas that I think could be improved. The first I have discussed in the review of the companion paper. The key thing that was difficult to evaluate in this paper was how sensitive the model result was to the key assumptions. In particular, how sensitive the model result was to the parameter rich calculation of FCI and the assumption that any sediment that is produced is frost susceptible. There is some discussion of the frost susceptibility problem in this paper, but it is quite vague and to my mind is not particularly convincing.

R-4.1: We thank the reviewer for the constructive comments. We understand that it is difficult to evaluate the sensitivity of the results to the assumptions and input parameters. It is a challenge to fully overcome this issue, because the FCI model is, as the reviewer correctly notes, a parameter-rich model. However, we tried to tackle this by focusing in this paper on transport-limited problems. Unlike the FCI, rates of frost creep are much more robustly linked to physics, and the creep model has fewer parameters. We note that frost heave is controlled by the parameter beta, and the diffusivity scales linearly with this parameter as shown in the companion paper. Hence, if the sediment is less frost susceptible, perhaps due to coarser grains, then beta will be smaller and creep rates will decrease uniformly across different combinations of sediment thickness and mean annual temperatures. We have rewritten section 4.1.1 in an effort to clarify the results' sensitivity to frost susceptibility of the sediment. In order to explore further the sensitivity of the results to assumptions affecting FCI, we have repeated experiment 1 using some of the different FCI patterns obtained in the companion paper and drawn comparisons via a new figure. The different FCI patterns reflect differences in how frost cracking is limited by water availability. This approach increases the consistency between the two manuscripts. It furthermore demonstrates that, while the steady-state sediment thickness is a reflection of the different soil-production curves, the fundamental form and development of summit flats remains a robust outcome.

C-4.2: Given the empirical nature of most periglacial geomorphology at the moment (and therefore much of the community that would like to digest your results), I think the authors could strengthen the paper considerably by attempting to place their model results in the real world. My suggestion would be to test the final model results against topographic data. As someone who has stood on summit flats without measuring their topography, I had the following questions: are these flat summits really parabolic? Is the scale of the relief and slopes that you calculate consistent with the development of these surfaces? I really like the warm and cool results, can you compare these to summit flats along a latitudinal transect, possibly through Scandinavia from Svalbard to Denmark or something similar? Because I think that the theory proposed here could be very influential in this field, and appealing to the empiricists and field geomorphologists could help to broaden the scope of the conclusions.

R-4.2: We agree that testing the model output thoroughly against topographic data would be a strong study. However, we think that this should be done systematically across many scales, and we see this as a next step for future work. Here we focus on presenting the conceptual model. We note that the new Fig. 1 provides examples of the landscapes motivating our model experiments, and we think that this has improved the link between the models and the natural world. See also response to comment C-3.2 by S. Brocklehurst.

C-4.3: It maybe outside the scope of this paper, but while I think summit flats are interesting, but the theory could be strengthened if the explanation could be extended to why periglacial processes aren't more efficient at mowing down peaks in areas of higher uplift rates (e.g. the "Teflon peaks" of the Chugach-St Elias Mountains and elsewhere). You do this a little in experiment 2, but could you crank up uplift rate and see what happens?

R-4.3: We agree that this is an interesting question, although we also feel that addressing this in a satisfactory way is beyond the scope of the present study. We have further emphasized in the first part of section 4âĂŤas others have noted before usâĂŤthat the temperature dependence of frost cracking allows high and cold peaks to escape the window of efficient frost cracking, particularly if they are too steep to develop extensive



regolith covers.

C-4.4: P7 L5: How do you determine the magnitude of the free scaling parameter. This seems really important, but difficult to constrain.

R-4.4: We cannot determine the magnitude of ke, because it is not a physical parameter. Thus, because we do not know its magnitude, we performed a sensitivity analysis (section 3.1.2) in order to explore how variations in ke influence our results.

C-4.5: P9 L25: Why not vary temperature across the surface? I assume it is because at 200m of relief it represents only 1.2 degrees of temperature difference in the model.

R-4.5: We do not vary the temperature across the surface in experiment 1 because we wish to keep the experiment as simple and clean as possible. A constant temperature enables us to study relations between temperature and average erosion rate and sediment thickness as documented in Figs. 4 and 6 (numbers refer to revised manuscript). We increase stepwise the complexity of the experiments and include spatially varying temperatures in experiments 2 and 3.

C-4.6: P10 L3: Why do you set ke to that value?

R-4.6: The ke value is chosen in order to yield a transport-limited scenario; see R-4.4 above concerning the sensitivity analysis in section 3.1.2. In the discussion version of the manuscript, the reader did not know of the sensitivity analysis at this point in the text, so we have now added a clarifying sentence.

C-4.7: P12 L15: Yes, but not with the frost susceptibility of the soil.

R-4.7: The sediment flux scales linearly with the frost susceptibility of the soil (parameter beta). This information and the specific link to beta have been added.

C-4.8: P12 L19: ke is not know, nor is the length scale of the penalty, nor the "flow resistance" parameters that you have introduced.

R-4.8: Good point. We have added this information to the sentence. See also response to comment C-4.1 above.

C-4.9: P20: This is the section that to me is the most unconvincing in terms of physics. (1) Beyond the Chamberlain paper there is a large literature that defines the grain size conditions required to produce frost heave, again I suggest the Harris papers as a good place to look at how even small differences in grain size, temperature, and slope can have large changes in the rate of downslope sediment transport. (2) The argument gets lost a little here. You state that frost cracking is unlikely to produce fine grained sediment. So then you introduce other processes (that you have not modelled) that may be acting just as fast on these slopes. These processes are somehow create a fine grained matrix below the open blocks of the felsenmeer. (3) Then you state that the fine grained matrix of the felsenmeer is frost susceptible, however do not provide any reference to either the grainsize distribution or whether frost creep rates have been measured here. I would rethink this discussion more carefully to make it more convincing.

R-4.9: The point of this discussion section is simply that frost creep depends on processes that can reduce grain sizes enough to make them frost susceptible. If frost cracking fails to do so, then erosion and landscape evolution may end up being limited by the rate of other (e.g. chemical) weathering processes. We surmise that the original text was not sufficiently clear on this point and therefore have modified it. Furthermore, an explanation of grain size effects has been added to the companion manuscript.

C-4.10: P22 L5: Lowering of the rates won't affect the conclusions, but they will affect the timescales of development of these landscapes. As you are running this across a specific timescale, how much does a lowering of the diffusivity affect this conclusion?

R-4.10: We have strengthened this part of the discussion by summarizing the consequences of transport-limited conditions: That sediment diffusivity sets the pace and thus also controls the total amount of erosion within a given timescale.

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